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Chapter Author: Richard R. Nelson

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Issues and Suggestions for the Study of Industrial Organization in a Regime of Rapid Technical Change

Richard R. Nelson
Yale University

My assignment is to consider the treatment of technical change in the industrial organization literature and to discuss how I think the facts and goals of technical advance should impinge on analysis of industrial organization. Since the literature has been surveyed in several recent books, I will concentrate on the second part of my assignment—key issues that require rethinking and research.[1] I shall be concerned particularly with problems in economic theory—the basic conceptual frames that researchers in the industrial organization field have to work with. My remarks will be focused on three main topics: first, the firm as an innovating and adaptive organization; second, the operation of market competition and other (including nonmarket) command and control mechanisms in a dynamic environment; third, some problems of public policy in sectors and situations where technical change is important. In all of these areas I will be crudely summarizing, and anticipating, ideas that Sidney Winter and I are developing.[2]

[1] See for example E. Mansfield, *The Economics of Technological Change,* New York, Norton, 1968; R. Nelson, M. J. Peck, and E. D. Kalachek, *Technology, Economic Growth, and Public Policy,* Washington, Brookings, 1967; and the relevant chapters in F. M. Scherer, *Industrial Market Structure and Economic Performance,* Chicago, Rand McNally, 1970.

[2] Some of the discussion rests heavily on earlier work. See Winter's "Economic Natural Selection and the Theory of the Firm," *Yale Economic Essays,* Spring 1964, and his "Satisficing, Selection, and the Innovating Remnant," *Quarterly Journal of Economics,* forthcoming. See my "Uncertainty, Learning, and the Economics of Parallel R & D Projects," *Review of Economics and Statistics,* November 1959; "A Diffusion Model of International Productivity Differences," *American Economic Review,* December 1968; and Nelson, Peck, and Kalachek, *Technology.*

Note: The author is indebted to M. J. Peck and R. E. Evenson for helpful comments, although they are implicated in no way. Sidney Winter is responsible for the good ideas.

THE FIRM AS AN INNOVATING AND ADAPTIVE ORGANIZATION

The theory of the firm exists on at least two analytic levels. At the formal level the theory postulates a set of rather simple characteristics of an archetypal firm. The formal theory rests on a deeper body of thought, which I shall call "appreciative" theory, and which attempts to structure qualitative notions about the nature of the firm and its activities in a manner generally less rigorous but richer than at the formal level. The theory of the firm at the simpler, more formal, level has a sharper analytic cutting edge than appreciative theory, and is more capable of generating, or proving, implications. However, the premises and arguments used to specify and justify the formal models rest on appeal to the more basic appreciation of the firm. Further, much of applied research in economics is guided by the appreciative theory at least as much as by the formal theory. This certainly characterizes much of the research in industrial organization. It is my contention that many researchers in the industrial organization field are working with an appreciative theory that is quite different from that underlying our formal textbook models. They recognize this and somehow feel guilty about it.

To put it bluntly, I do not think that the traditional theory of the firm is adequate for analysis of industries in which technical change is important. I think that the appreciative theory of the firm used by industrial organization economists is better than the appreciative theory of the full-time theorist and, further, provides a good basis for formal theory of an interesting and useful sort. The points I will make abut on the long-standing debate about the theory of the firm—behavioralism, managerialism, and so forth—but perhaps even more they are Schumpeterian. I will begin by questioning our traditional theory of the firm at the appreciative level, and then go on to ask some questions about what it is legitimate to assume about firms in the simple, formal models used in the theory of industrial behavior.

In traditional appreciative theory the firm is viewed, first of all, as a *unit;* I will not argue about this point here, but some of my later remarks are strengthened if one recognizes that within the firm there are many people and suborganizations that must somehow be organized. Second, the behavior of the firm is viewed as subjectively rational, in the nontrivial sense that the firm has some objectives in mind and some rather firmly held reasons for doing what it is doing (at one extreme calculations, at the other arguments based on experience). The firm's

behavior is viewed as objectively rational in that it would not be trivial for an economist who understands the decision problem to find significantly better policies for the firm than those being chosen.[3] Third, the firm is viewed as being able to operate a variety of technologies reliably and efficiently, subject to the constraint of availability of the necessary inputs (including machinery, skills, etc.). However these constraints are assumed to be not particularly binding over the time period relevant to the analysis. Hence the firm is viewed to a first approximation as being able to employ effectively any technology that any other firm can. I have asserted these elements of appreciative theory in a drastically terse way, while in fact the theory is laden with complexity, nuances, qualifications, and exceptions. I maintain, however, that this is a fair characterization of those aspects of the theory to which we appeal in constructing more formal models.

Once one begins to move from appreciative to formal theory this vision of the firm leads naturally to a model that assumes firms maximize some objective (the deeper theory does not necessarily imply profit) subject to the constraint of a production function and demand and supply equations. Since subjective and objective maximization are the same, the firm can be expected to behave according to the optimizing rules the economic analyst computes. The deeper model almost suggests that all firms are pretty much the same or, rather, provides no reasons why they should be different, and in the absence of special reasons for postulating differences in technological capabilities, access to markets, or of motivation this generally is what we end up assuming

[3] The "subjectively rational" concept means different things to different people, but almost everyone would rule out basically random behavior. Although Alchian and Becker attempt to show that even in this case some of the theorems go through, their proposals do not seem intended as a serious assertion about the nature of firm behavior (Armen Alchian, "Uncertainty, Evolution, and Economic Theory," *Journal of Political Economy,* June 1950; Gary Becker, "Irrational Behavior and Economic Theory," *Journal of Political Economy*, February 1962). At the least some kind of consistency of behavior is expected. And most economists would assume that this consistency is purposeful and is the result of some thought, rather than being purposeless, mindless rigidity (although some use of rule-of-thumb behavior would not be totally excluded). The "objectively rational" point is different, and important. Despite Machlup's earlier insistence that the firm's optimization must be considered as subjective, two things are clear. First, most economists assume that the firm's perception of the world has some contact with reality; firms are viewed as competent—a point I shall develop shortly. Second, in the formal theory the economist plays God, and on the basis of his assessment of what is *objectively* rational, makes predictions as to firm behavior.

in the formal modeling. This is convenient because then we can proceed with the business of modeling industry behavior on the basis of appeal to a typical firm. Later on the theory generates various survival arguments that can be invoked to justify this assumption.

We end up with a theory which views the firm as a competent clerk. This is so both in main-line positive theory and in normative theory. Firms carry out certain well-defined, widely known activities, using generally available resources, picking the activities and their levels according to well-defined, easily computable (and optimum) decision rules. In positive theory this characterization exactly fits competitive theory under the special case where all firms (including the potential entrants) possess the same production sets. It is slightly inaccurate when applied to oligopoly theory where firm differences in production sets, supply conditions, and reaction functions are admitted in some models, or to monopoly where the monopolist is *de facto* unique. But the theory still gives the impression that one set of oligopolists, or one monopolist, is pretty much like any other. In normative theory also the characterization exactly fits the analysis of the optimality properties of competitive equilibrium (with some awkwardness creeping in regarding oligopoly when considering research and development behavior), but the image of the "interchangeable clerk" is strong throughout. This image of the firm stems from our proclivity in our theory to take the technologies, resources, and demands as given. Thus the economic problem is to get the job done efficiently. Bread and automobiles are to be produced in the right quantities and in the right ways given the preferences, resources, and technologies available to the economy. (Let me ignore the question of distribution.) A competitive market provides clear signals as to what is to be done; following the signals is a straightforward business.

This is a plausible characterization of parts of the economic problem and might be a good overall characterization (with appropriate market failure caveats) in a world of no real change; for example, the circular flow world of Chapter I in Schumpeter's *Theory of Economic Development* where "the data which have governed the economic system in the past are familiar, and if they remain unchanged the system will continue in the same way." [4] This is also a world in which a variety of plausible learning mechanisms vitiate the arguments that "tech-

[4] J. Schumpeter, *The Theory of Economic Development,* New York, Oxford Paperback, 1961, p. 81.

nological knowledge is not a public good" and "maximization is difficult if not impossible," and in which Friedman-Alchian evolution-survival arguments seem to make sense (with some important caveats that I will not discuss here).

The circular flow concept, with mechanical, interchangeable firms, probably can keep its footing, if shakily, in a world of smooth, predictable change—such as one with exponentially growing factor supplies and consequent changes in demands. In some models technical change is treated consistently with this view. Indeed, Schumpeter himself, in his *Capitalism, Socialism, and Democracy,* talks about the "routinization of innovation" thus bringing technical change back into his, now dynamized, circular flow model.[5]

However, even if technical change, and adjustment and accommodation to it, can ultimately be routinized, this certainly has not occurred yet.[6] Innovation is inherently creative and personal. In the world of Schumpeter's Chapter III:

> While in the accustomed circular flow every individual can act promptly and rationally because he is sure of his ground and is supported by the conduct, as adjusted to this circular flow, of all other individuals, who in turn expect the accustomed activity from him; he cannot simply do this when he is confronted by a new task.
>
> Carrying out a new plan and acting according to a customary one are things as different as making a road and walking along it.[7]

Economic theory simply has not grasped this distinction. Perhaps the most apparent and striking failure of theory is the proclivity to treat research and development as merely another form of investment, with, perhaps, an unusual amount of uncertainty. But this statement, at the appreciative theory level, just does not characterize adequately the kinds of experimenting, error making, partial correcting, and insightful or blind behavior that seems to go on in major R and D. Nor does it appear to be an adequate general characterization of firms that are trying to do things they have not done before, even though other firms have. Recall

[5] Particularly Chapters 11 and 12.

[6] It would be pedantic to cite many references here. But consider, for example, the case studies in J. Jewkes, D. Sawers, and R. Stillerman, *The Sources of Invention,* New York, St. Martins, 1958; and in J. Marschak, T. Glennan, and R. Summers, *Strategy for R and D,* New York, Springer Verlag, 1967.

[7] Pp. 74 and 85.

Henry J. Kaiser's unsuccessful attempts to master the automobile business.[8] Firms fail, and succeed. Our positive theory at the present time does not seem to have room for the kind of purposive but groping behavior that seems to characterize the operations of firms in a regime of rapid technical change.

Nor does our normative theory adequately deal with this. It is clear that in many important sectors and situations not only is innovation important, but it is an important part of what we want firms to do. To hit this point hard let me shift focus here from the implicit context of private goods and markets to the public sector, and broaden the concept of firm to include organizations of unspecified legal form. In the traditional public finance literature the task of the public bureaucracy, plus contractors, is viewed as analogous to the task of the firm in competitive theory—carrying out activities to provide public goods and, more usually, services. Yet a large share of the important programs are better viewed as efforts to solve problems, where the solution is likely to require new hardware, or a new way of doing things, or a new program, and, hence, "innovation" by the standard definition. Project Apollo is the most striking example. Much of what we are trying to achieve in defense procurement also is hardware innovation. Or, consider the War on Poverty, where what we are mainly trying to do is find, and then implement, programs that will work rather than operating existing programs (which are felt to be unsatisfactory).

I shifted to public sector activity because here it is easier to see that quite often what we are asking the organizations to do is innovate, rather than to meet a well-specified demand in an efficient and well-known way. Yet clearly this also characterizes what we expect and get from firms in a large number of private goods, market-organized sectors. While we hear more than enough about "progress being our most important product," as theorists we have refused to absorb any of this. Robert McNamara's statement is a bit flamboyant ("What in the end is management's most fundamental task? It is to deal with change. Management is the gate through which social, political, economic, and technological change—indeed change in every dimension—is rationally and effectively spread through society" [9]), but we do have to get at least some of this flavor into our theory of the firm.

[8] See "Arrival of Henry Kaiser" and "Kaiser-Frazer, Roughest We Ever Tackled," *Fortune,* July 1951.

[9] Remarks made at Millsaps College, Jackson, Mississippi, February 24, 1967; reprinted in J. J. Servan-Schreiber, *The American Challenge,* New York, Atheneum, 1968, p. 76.

The present main-line appreciative theory has no real room for this dynamic concept, and industrial organization economists long have known intuitively of this deficiency. In an environment of rapid technical change it is implausible to describe behavior in terms of concepts like "subjectively rational"—except perhaps in the trivial sense that the firm is trying to do as well as it can, has some clues as to appropriate behavior, and if it clearly saw ways of doing better would be doing them. But one would expect to find firms often having neither articulate reasons nor appeals to experience to justify what they are doing, and indeed being somewhat nervous about it. It certainly seems inappropriate to view behavior as being objectively rational in any nontrivial sense. In particular there is no case for the assumption that the firm will behave according to the rules the economist calculates as optimal. For obvious reasons it seems a bad misspecification to assume that a firm has access—over the relevant analytical period—to any technology to which any other firm has access. For all of these reasons there is no justification for sliding into the notion of a typical firm in a dynamic environment. Indeed what appears important is that individual firms are unique. In short, the firm cannot be viewed any longer as a competent, easily predictable, interchangeable, clerk working in a well-structured environment on well-defined tasks. Rather, the firm must be viewed as attempting to keep its footing and to make progress in a poorly structured, changing environment by trying and doing appropriate new things.

At the level of appreciative theory, how should we characterize a firm, ideally in a way that is consistent with the traditional perspective where that is appropriate? Let me appeal here to the literature on organizational theory and the behavioral theory of the firm for justification of a presumption that, whether as the result of rational analysis or not, the firm at any time operates according to a set of decision rules that links environmental stimuli to responses by the firm.[10] In the traditional theory it is analytically convenient to denote some aspects of these decision rules as technological, and distinguish these from others which can be characterized as higher-level decision rules. There are some serious difficulties with this clean split, but I will not go into these here. In any case the theory of the firm aims for a convenient, and as simple as possible, characterization of these decision rules. If this can be deduced from, or assumed to be the result of, "maximization" it may be convenient, but it is not necessary to the theory as long as the analyst can

[10] See R. Cyert and J. March, *A Behavioral Theory of the Firm,* Englewood Cliffs, N.J., Prentice-Hall, 1963.

specify the rules somehow. Indeed, a perfectly viable theory would simply declare the existence of these rules and certain aspects of their form, and that they are stable and constant. This really is much of what the maximization theory does. All that the maximization connotation accomplishes is to make the specification plausible.

In the traditional theory these decision rules, both higher order and technological, are viewed as capable of invoking a wide range of firm responses to a considerable domain of environmental stimuli—prices, etc. This is what makes comparative statics work. Let me again appeal to the organizational literature to suggest that, rather, we should assume that the built-in decision rules of a firm apply to only a small domain of environmental conditions and are capable of invoking only a limited range of responses. Put another way the firm at any time commands only a small set of activities and has thought through responses to only a limited range of market contingencies. This, it seems to me, should be an explicit part of the theory.

The model of the firm needs two dynamic components. One is specification of what determines the expansion or contraction of the firm (the level of employment of the decision rules it is using). In other words, the theory needs a submodel of widening investment.

In addition there needs to be an analysis of mechanisms that will induce firms to change their decision rules. The assumption that the firm's decision rules at any time are limited and simple means that in an environment of change, either of external market conditions or of perceived technological possibilities, the firm often will find itself in situations where its built-in rules are, or are felt to be, inappropriate. In our analysis of the process by which firms change their decision rules (perhaps higher order as well as technological) it seems important to be much more sophisticated than we have been about modeling two different, although far from independent, kinds of mechanisms. One essentially is the processes of assessment and search that are largely internal to the firm. An obvious example is research and development, but I also would include operations research, market analysis, management contemplation, etc., where the firm is scrutinizing its own operations and searching for ways to improve them. It seems useful to me to distinguish these internal assessment and search processes from another, undoubtedly linked, class of activities focused on the conduct of other firms. In this latter class the firm is looking to sources of improvement by examining the behavior of other (presumably successful?) firms. While the internal search and the external scan mechanisms clearly should be re-

lated at the level of appreciative theory, at the formal level the first class can be viewed as generating innovations (not necessarily improvements); and the second class, diffusion models. While the purpose of these activities is to improve performance I think it would be a grave mistake to assume that they do so reliably. Nor does it seem appropriate to assume that these mechanisms are working all the time on the full range of firm activities and procedures. Indeed, characterizing what things capture the attention of the intelligence mechanism and "turn it on," and the nature of the search process would seem to require theoretical delicacy, and much empirical investigation.[11] Clearly firms differ in these characteristics.

The explicit recognition that many of the decision rules, perhaps particularly the technological ones, are subject to more than very occasional change reduces the attractiveness of a theory that appeals to stable decision rules. I would like to propose, however, that in an environment of rapid change where the lower-order rules may be quite unstable, one might hope to find more stability in the qualitative "meta-rules" that guide changes in the rules. Thus, one might well be able to identify and describe the intelligence mechanism of a firm, its R and D style, and the broad strategy that guides its search for improvements. These surely are more difficult to describe in a simple way than the kinds of rules that have been uncovered on, for example, pricing. But at

[11] One obvious characterization is the "satisficing" model which, in a stylized version, assumes an on-off switch mechanism linked to the performance of the firm relative to its "aspirations" level, and an incremental search starting in the neighborhood of existing practice. Contrary to many complaints about this characterization it certainly does seem a basis for rigorous formal modeling. However, it seems inconsistent with the practice of highly profitable firms to continue to do considerable R and D; it does not adequately model in either the "switch" or the "search" sense the looking to other firms that seems to characterize "diffusion" processes, and it seems unable to account for "major" innovation.

Clearly there are much more sophisticated models of attempted rationality than the simple satisficing model. What is required of theory, I suggest, is that the model not require the decision maker to know more than the model shows he can find out, and that the costs of information gathering and processing be considered at least implicitly. W. Baumol and R. Quandt make some of these points in arguing for the rationality of rules of thumb ("Rules of Thumb and Optimally Imperfect Decisions," *American Economic Review,* March 1964). In several of his works Stigler has generated some very interesting deductions from models that are explicit about the processes by which information and "clues" get acquired. And, of course, J. Marschak has been making some of these points for years ("Theory of an Efficient Several Person Firm," *American Economic Review,* May 1960).

the level of appreciative theory it does seem plausible that firms can be characterized in these dimensions in an illuminating way. Further, it seems plausible that it is at this level that we can find and characterize the sensible response to change characterizations of firm behavior (for example, if wage rates rise significantly, search for ways to cut down use of labor) that we work so hard to deduce from our optimization models. One does not need an optimization model to predict sensible behavior.[12]

Some industrial organization economists, writing about important firms in industries characterized by rapid technological change, have in fact been applying something like this kind of an appreciative theory. They have been digging into and trying to characterize pricing policies and investment rules, without really trying to deduce these from optimization assumptions. Differences among firms have been a matter of some interest to researchers. In some of the literature there have been attempts to characterize the R and D philosophy of a firm or its overall strategy.[13]

Thus the nontraditional appreciative theory apparently meets the test of serving as a useful framework for empirical investigation. However, one cannot rest comfortably with an appreciative theory unless one sees what a formal theory consistent with it would look like. In the first

[12] Note that "neoclassical" implications of a wage increase probably can be deduced even from a simple satisficing switch, incremental search model. The wage rate increase decreases profits which (if they were "normal" before) flips the search switch and improvements will be found on the capital-intensive side of the existing factor mix decision rule. Note that the larger the wage increase the larger the substitution that will be generated (under plausible assumpions) before target profit levels are again achieved.

Note also an "asymmetry" (perhaps realistic?) of this mechanism. A fall in the price of capital will not flip the search trigger. A "never completely off" switch assumption seems necessary to assure neoclassical results in this case.

Note also that it will take time before the new equilibrium will be found and, depending on one's specifications, there will be costs of searching and perhaps mistakes. I take it that there is increasing interest among theorists in treating adjustment lags and costs explicitly. By and large the justification has been in terms of expectations or friction. The kind of explicit search model I have been discussing seems richer.

[13] See, for example, Cyert and March, *Behavioral Theory,* Chaps. 7 and 10; and A. Kaplan, J. Dirlam, and R. Lanzellotti, *Pricing in Big Business: A Case Approach*, Washington, Brookings, 1958. On the question of corporate strategy see Alfred Chandler, *Strategy and Structure,* Garden City, New York, Doubleday-Anchor, 1962; and Neil Chamberlain, *Enterprise and Environment,* New York, McGraw-Hill, 1968.

place, while appreciative theory is inherently somewhat fuzzy, having and working with a formal theory serves to keep the fuzziness within bounds, and to sharpen the appreciative theory. Second, as will be elaborated shortly, the theory of the firm is mainly used as a component of the theory of industrial behavior, in which a more concise, formal, and manipulable model of firm behavior is needed. Thus it seems important to develop a formal theory of the firm consonant with the appreciative theory sketched above.

What is required is a formal theory of firm behavior that is consistent with traditional appreciative theory when appropriate, yet is also capable of modeling the innovative and adaptive firm where that is appropriate. The guidelines are clearly specified in the appreciative theory. The firm at any time should be described by its size and the decision rules it is following. These rules determine whatever endogenous variables the theory aims to explain as a function of a variety of external variables. The firm also needs to be characterized in terms of its expansion and contraction rules and, to anticipate the theory of industrial behavior, the conditions that would trigger entry of a firm that is not in the industry should be specified. Several models of this sort already exist.[14] However, for a model really capable of generating and responding to technological change, it seems essential to incorporate the two kinds of learning processes discussed above; some kind of an innovating or internal search mechanism for improvement, and some kind of an imitation mechanism whereby what one firm does can induce another firm to do likewise.[15] A variety of specifications might be employed. However, it seems essential that at least the innovation generating mechanism not be specified as objectively rational.[16] The burden of prediction that the system move in an objectively rational direction should rest on specification of the search mechanism, on the diffusion machinery, and on responses to market pressure. It would appear that such a theory can be built, and is capable of generating some interesting and plausible implications. The merit of such a formal theory, as sug-

[14] The most elegant model of this sort is Winter's "Satisficing, Selection, and the Innovating Remnant." But several of the stochastic growth models are similar in many respects. See, for example, E. Mansfield, "Entry, Gibrat's Law, Innovation, and the Growth of Firms," *American Economic Review*, December 1962.

[15] Winter's model does have a simple innovation mechanism.

[16] My insistence on this point is stronger than that we must have room in our model for autogyros and Edsels. It has to do with the whole way we look at the technical change process. I will elaborate what I mean in the last section of this paper.

gested above, is mainly to be found at the level of our theory of the industry, to which I now turn.

DYNAMIC MARKET COMPETITION AND OTHER FORMS OF INNOVATION: GENERATING AND SELECTING ENVIRONMENTS

Economists, particularly industrial organization economists, seldom are interested in the behavior of particular firms, but rather in the behavior of industries or sectors. The sector usually, but not always, comprises a number of firms whose behavior cannot be assumed to be independent. Further, the dimensions of sector behavior in which we are most interested usually involve, in an essential if often summary way, specification of what is going on *outside* the particular group of firms that constitutes the sector. We have a tradition of viewing firms as means, not ends. Thus, in our theory of industrial behavior we are concerned with the way in which demands for the output of the sector get generated, and the extent to which the sector satisfies these demands. We also have an appreciation of general equilibrium considerations even in our partial equilibrium analysis. Thus, we are concerned with the costs of operating the sector at various levels and ways, the extent to which the sector operates to minimize real costs at any level of operation, and the way it balances marginal benefits and costs.

Therefore, in conceptualizing at the industry level we generally employ a greatly stripped down and simplified theory of the firm. In addition to specification of the characteristics of firms, our theory of the industry or sector, at both the appreciative and the formal modeling level, involves specification of the environment within which firms operate. The "market" in traditional theory is a model of such an environment which determines the signals, incentives, and constraints which impinge on firms and thus on their behavior. In the traditional theory the environment is determined by two classes of factors. One is the behavior of the outsiders, particularly those who demand the good or service the firms in the sector can provide, and those who supply inputs which have alternative uses or values. The other is behavior of the internal system taken as a group—the competition that goes on among the individual firms. Thus the market is at once a connecting link between demanders and suppliers of both products and inputs, and a constraint upon the behavior of the insiders, in short, an apparatus of command (through effective demand) and control (through competition). There are many

other kinds of command and control structures, such as those that characterize primary education, medicine, or the foreign policy establishment. Now, in speaking of industrial organization, I assume that "organization" refers to the command and control structure and that although we tend to concentrate on markets (just as we have tended to concentrate on firms which aim for a profit) the subject matter of industrial organization in principle includes nonmarket command and control structures and organizations with objectives defined in terms other than profit.

I make these more or less obvious remarks so that we can be clear that the traditional appreciative theory of industrial behavior in a market environment is a special case. In the traditional theory the signaling and incentive generation mechanisms are modeled as well-perceived product demand and factor supply curves. The internal control environment is deduced from the condition that *no* firm can improve its profit conditions. Clearly our modeling of sectors which are not controlled by the market would be somewhat different. However, our analysis of market and nonmarket sectors has been dominated by notions of steady state equilibrium associated with our notions of firms as clerks working in a well-defined and relatively constant environment.[17]

The discussion in the preceding section suggests that this positive theory does not adequately characterize the environment of firms where technical change is rapid. The assumption of a well-perceived demand curve for product or supply curve for input is plausible only if one can describe mechanisms whereby these curves in fact get well perceived. This would seem to imply considerable experience on the part of the firms in the industry in the relevant environment of demand and supply conditions. This clearly cannot be assumed in an environment of rapid change in either demand or supply conditions. In particular, it seems completely implausible in considering the demand for a major innovation. Nor under these conditions does it seem plausible to model the environmental constraints in terms of industry equilibrium for that is not where the action is going on. If the industry or problem we are concerned with looks like one in which we can expect change in the equilibrium conditions that is rapid relative to the speed with which equilibrium is approached, or even in which one doubts that equilibrium (perhaps

[17] The various "voting" models clearly are in this spirit. It turns out that in these models very often an equilibrium does not exist. But this analysis—the way the problem is set up—is virtually identical to the setup for analysis of market equilibrium.

constant) will be closely approached during the relevant time interval, one should not play equilibrium games. Rather one has to work with an explicitly dynamic model of firm and industry behavior. The competitive environment of any firm is provided by the others moving toward equilibrium, but not by their presence there.

The problem is not just in positive theory as a framework for description and explanation; it is in normative theory as a framework for evaluating performance. If doing things better is a good part of what we are trying to call forth, the market cannot be conceived of strictly as a mechanism to "control clerks" (which is the image of Langian socialists as well as of neoclassical economists who believe that actually having competition may be easier than getting the decision rules of competition followed in the absence of real competition). Rather the market has to be viewed as a mechanism stimulating new mutation (innovations) and doing a creditable job of somehow discriminating between the good and the bad, spreading the former and killing the latter. Even in an environment where rapid technological change is occurring and is highly valued, this is far from all that we want from a market control system. In addition we want that system to stimulate and enforce the neoclassical virtues of economic efficiency, i.e., achieving an appropriate level of output and a minimum economic cost. But since these are going to be changing over time, here, too, market control must be viewed in terms of stimulating moves in the right direction.[18]

Again let me focus on public sector activity to hammer home the point—as well as to introduce a policy issue that I will treat in the following section. The 1960's marked the burgeoning of interest in systems analysis (or cost-benefit analysis or any of a number of titles) as a tool for governmental decision making. Thinking of the decision maker (the systems analyst? the Cabinet Secretary? the President?) as commanding a bureaucracy under him led to a sharp split between the public finance literature, where demands (decisions) automatically were fulfilled, and the industrial organization literature, where demands had to draw forth responses by impinging on a (market) environment of potential suppliers. As experience has accumulated, the clean lines that once used to exist between industrial organization and public finance have been destroyed. There has been growing appreciation that getting the program

[18] I state these hackneyed points here not for novelty value but simply to point out that most of contemporary formal theory continues to ignore them. There are exceptions, William Nordhaus's recent book, for example, *Invention, Growth, and Welfare,* Cambridge, Mass., M.I.T. Press, 1969.

performed (the demand met) required the appropriate responses on the part of a variety of organizations, public and private. It became increasingly apparent that this was no trivial requirement.[19] Getting the education or health industries to do what the federal government wants it to be doing turns out to be extremely hard. Part of the difficulty here is that the federal government is only one of many who are trying to get the system to do what they want. But President after President has found it difficult if not impossible to get the State Department to do what he wanted.[20] The point I am trying to make is that having a well-working command and control structure over a group of firms is no trivial matter and that nonmarket sectors have the same command and control problems as the market sector.

However, note that to a considerable degree where the nonmarket sectors seem to be falling down is in effective adaptation to change—technological and other. The education sector has failed to develop appropriate responses to the rise in teacher salaries which, we had hoped, would generate some effective search for ways to increase the pupil-teacher ratio through increased capital intensity or more efficient techniques of teaching. It has failed abysmally to respond to the changing nature of the demands put upon it, largely learning how to educate poor children with non-middle-class values, but also how to educate bored middle class youngsters and how to operate integrated schools. Similarly the health sector has not learned to respond to rising physicians' salaries and fees, and the changing nature of demands put on it.

The neoclassical allegory does not seem to characterize these and, I suggest, most other important kinds of responses to changing factor prices and demands that we want of an economic sector and get out of some. As stressed in the first section, that allegory implies much more complex decision rules keyed to a richer domain of possible external situations and range of responses than we have any reason to assume. For large changes in, say, relative factor costs or demands, I do not think that we can assume firms have a response already thought through or that they can think through to a response *ex ante* that is subjectively and objectively rational. Rather the response has to be considered as an innovation which may or may not turn out to be really economic or really responsive.

[19] See, for example, the last chapter of Charles Schultze's *Politics and Economics of Public Spending,* Washington, Brookings, 1969.

[20] For a very interesting discussion of bureaucratic versus optimizing behavior see Graham Allison's "Conceptual Models and the Cuba Missile Crisis," *American Political Science Review,* September 1969.

Some evidence on this and some implications for the theory of markets and other forms of command and control structures are provided by what has happened to the perception of systems analysis over the past few years, particularly in domestic programs. I think it fair to say that in the mid-1960's there was a faith that with good analysis we could reliably choose *ex ante* among alternative programs on the basis of data gathered and analysis done, even though these programs were in large part untried and the demands had never before been adequately met. We felt we could do this without actually really observing the alternatives in action. In effect the faith here was closely analogous to the economic theorist's allegory about the wide range of choices and circumstances over which the firm can make rational choices *ex ante*. As experience accumulated it became clearer and clearer that there seldom was sufficient information *ex ante* to make reliable bets, and that at the least *ex ante* analysis had to be complemented by *ex post* evaluations. More recently of course thinking about rational policy development has moved more and more toward conscious experimentalism, with the role of the analyst seen as that of setting up a number of experimental programs to obtain data and to try them out, and then on the basis of later data generated in the course of the program, selecting or modifying the menu of alternatives.[21] In short, the model of how public programs should be chosen has moved from the rational choice *ex ante* paradigm to a paradigm which explicitly recognizes that the problem is that of trying out new things, and getting appropriate feedback for screening and selection.

There is no reason to believe the situation is much different in market sectors. While public sector industries seem to have unusual difficulties in selecting and spreading good innovations, in the private sector as well as the public it seems necessary to characterize dynamic processes and mechanisms of selection and diffusion in terms of a flow of innovations, many of which are no improvement at all. Traditional theory that relies heavily on equilibrium concepts seems to abstract from these phenomena and their implications. A good dynamic industry model, I suggest, incorporates a stripped down version of the theory of the firm proposed in the preceding sections. Many people have granted that a quasi-behavioral model is appealing as a model of a particular firm but

[21] For an interesting essay in advocacy see D. Campbell, "Reforms as Experiments," *American Psychologist*, April 1969. For a discussion on the context of the negative income tax see the articles by G. H. Orcutt and A. L. Orcutt, and by G. H. Orcutt, H. W. Watts, and J. B. Edwards in *American Economic Review*, September 1968.

have doubted whether it can be incorporated into an industry model. The claim here is that it can, indeed it is the natural model of the firm to use in a model which includes the possibility of dynamic competition. Firms are characterized by their technologies and static decision rules, and also by the way they generate innovations, expand or contract as a function of their profitability, and imitate (successful) innovations of others. What are the required components of a theory of command and control structure (competition) in an environment where rapid technological change is desired or occurring? The objective is to model demands and competitive pressures in a way that fits our proposed general model, that is consonant with traditional theory where that is appropriate, but which also characterizes more adequately a dynamic, changing environment where that is appropriate.

First, there has to be much more sophistication in modeling the demand for innovation. There are significant problems in positive modeling. It cannot simply be assumed that there is a well-perceived demand curve. One has to get a realistic specification of the speed with which consumers assess the plusses and minuses of the new innovation and of how, in turn, this affects the signals and profitability of the innovating firm. There also are some major normative issues. In a dynamic environment it is doubtful that consumers immediately assess accurately the properties of the new products—there are real issues to be considered regarding the effectiveness of consumer evaluation procedures. While economists increasingly are looking at problems of externalities, these would appear to warrant even more consideration in an environment where rapid change is occurring. There may be something to the argument that with enough time, forces of self-interest will cope with the externalities problem. However, the mechanisms that get externalities reflected in bargains and in incentives to producers cannot be assumed to work quickly. One would expect externalities to be rampant in an environment of rapid technical change.

Second, the dynamics of interactive behavior of the group of firms in the sector needs to be modeled quite carefully. The analysis needs to trace through the manner in which the responses of consumers to an innovation, and of the innovating firm to the success of its innovation, change the environment for other firms and in turn affect their behavior, which feeds back, etc.[22] The nature of the expansion and contraction

[22] For an interesting approach to certain aspects of this, but within a maximization context, see F. M. Scherer, "Research and Development Resource Allocation Under Rivalry," *Quarterly Journal of Economics,* August 1967.

and entry-and-exit behavior of the firms clearly is an important characteristic of the dynamic sector environment. In addition to asking the extent to which improved performance gets reflected in higher profit, one must ask how sensitive are expansion and contraction rates to profitability (using the term as a general proxy for whatever the organization aims for)? How sensitive are entry rates to the average profitability of established firms? Are there limits on firm size or (more saliently) on the extent to which particular firms can and will use a particular technology or innovation? To the extent that expansion rates are not particularly sensitive to profit or if there are sharp limits on ultimate size, the efficiency of dynamic response is deterred directly and, also, indirectly because (under plausible models) less pressure is put on the noninnovators. One is tempted to conjecture that sectors in which individual organizations are bounded geographically (schools?) provide a less dynamically stimulating environment than those in which growth of any particular organization is not closely bounded. On the other hand, if expansion mechanisms work quickly and powerfully and imitation mechanisms sluggishly, a successful innovator will become a monopolist. In any case it would seem that analysis of this kind of question is important in studies of any particular sector.

Successful innovations spread in part through growth of the innovators, in part through imitation. It is apparent that in market sectors both mechanisms are at work, although the relative importance of each does not appear to have been studied much and probably varies from sector to sector. It is important to note that the two mechanisms are not independent. In public or nonprofit sectors the "expansion of the innovator" mechanism is largely or totally scotched. This means that a desirable innovation cannot be spread without imitation. At the same time it means that little or no spur is put to organizations to adopt innovations; there is no build-up of competitive pressure on the public monopoly.

There are some compensating considerations. In particular, while the incentive to imitate is weakened when the innovating unit cannot or will not expand, at the same time there is under these circumstances no incentive for the innovators to try to deter imitation (in the private sector, for example, deterrence is the function of the patent system). Organizations that cannot expand, and that know others cannot either, have little to gain by preventing others from adopting their own successful practices. Much of the still remaining faith in the ability to diffuse successful innovations through publicly structured sectors, despite the

lack of any clear-cut profitlike incentive and despite the existence of sharp boundaries on organizational size, rests on a faith in the apparatus for generating imitation. However, we know precious little about diffusion mechanisms, and patterns of a sector should be a prime topic for investigation in studies of industrial organization in an environment of change.[23]

If one can assume that the speed of consumer response and strength of feedback to suppliers for better or lower-priced products is great enough, that expansion and contraction rules are sensitive enough to profit, and that imitation mechanisms work quickly and reliably relative to the pace at which innovations occur, then it seems reasonable to model the environment in terms of equilibrium conditions. But in a world of rapid innovation, one must pay explicit attention to the transients.[24] It does seem possible to develop a general model that is capable of generating competition either in the neoclassical sense or as Schumpeter described it, depending on what one assumes about key parameter values; and which kind of competition it is in any particular sector clearly makes a difference, both in terms of positive description and analysis, and in terms of the major public policy issues to watch out for.

POLICY ISSUES

In this concluding section I will discuss, in summary form, two major policy issues involving industrial organization in a regime of actual, or desired, rapid technological change. These are worth discussing not only for their own sake, but also for the opportunity they afford to develop further in a concrete setting some of the points made abstractly in the preceding two sections. The first involves issues that arise in trying to program very rapid technological advance in particular sectors. The second involves problems of generating, selecting, and diffusing innovation in public sector or mixed industries.

[23] There have been several first-rate economic studies. See, for example, the several chapters on diffusion in E. Mansfield, *Industrial Research and Technological Innovation,* New York, Norton, 1968. However, there has been very little solid work on "mechanisms."

[24] Obviously this is one of Herbert Simon's central points in his "Theories of Decision Making in Economics and Behavioral Science," *American Economic Review,* June 1959, and in many of the other papers he has written in criticism of the maximization theory.

Programing of Rapid Technological Advance

As remarked earlier, in *Capitalism, Socialism, and Democracy* Schumpeter presented the vision of a future world in which major innovation was routinized. In his *New Industrial State* Galbraith suggests that this stage essentially now has been reached in the large American corporations, and Servan-Schreiber takes a similar position regarding practice in the United States as a whole. The economist's standard model incorporating R and D likewise is consonant with this perception, treating R and D as basically an investment decision not unlike most others.

The theoretical restructuring proposed in the preceding section conflicts strongly with this point of view. In several places I insisted that the innovation process not be modeled as objectively rational either in the sense that outcomes can be closely predicted in advance or in the sense that outside experts (the economists?) would agree on the predictions. In this connection, I insisted that a good fraction of innovations are not improvements. In the industry modeling of technical change I rested considerable weight on the generation of a variety of innovations and hence on processes of *ex post* evaluation and selection.

This disagreement about the nature of the innovation process is important not only for modeling but also for policy. If one believes in the theory of the routinization of innovation—R and D as investment—then one soon is drawn toward looking to R and D, focused on particular national problems, as not just a promising but a reliable instrument for public policy. Further, belief in the reliability of the instrument naturally leads one to analyze in advance the range of alternatives, pick the one that looks best, and put his chips on it. If, on the other hand, one believes that R and D is extremely uncertain, one adopts a "let a thousand flowers bloom" point of view, sees R and D as an interesting, perhaps highly promising, policy instrument, but does not treat the instrument as reliable and, hence, hedges both by using other instruments and by spreading the R and D bets. The first approach leads to the Defense Department style of R and D, and to such forced-paced programs as the supersonic transport and the breeder reactor of the Atomic Energy Commission. The second perception leads one, in public sectors, to spreading of funds, such as is done by the National Institutes of Health, and in private sectors, to seeking to encourage a diversity of research and development, private as well as public.[25]

[25] Clearly, the discussion here harks back to the earlier work done by Burton Klein and others on military R and D. See his articles and also A. W. Marshall

The mutation-selection model seems much more consonant with history. One of the most striking impressions of the history of technological advance in most American industries is the diversity of sources. New products, processes, inputs, and equipment for an industry have come from many different firms in the industry, suppliers, purchasers, new entrants to the industry, outside individual inventors. Many developments that early seemed very promising did not pan out. Many important breakthroughs were relatively unexpected and were not supported by the experts in the field. While detailed histories are not plentiful, and many of these do not shed light on the question, one has the impression that in most of the technically progressive industries, like chemicals and electronics, most of the bad bets were rather quickly abandoned, particularly if someone else was coming up with a better solution, and good ideas generally could proceed along a variety of paths to get their case heard.

The military research and development programs since the mid-1950's, the civilian reactor program of the Atomic Energy Commission, and experience to date with the supersonic transport, are a sad contrast. In these areas the early batting average has been dismal, just as it has been in the domain of decentralized development. But there has been a proclivity to stick with the game plan, despite mounting evidence that it is not a good one. In areas where R and D was more decentralized and competitive, such persistence appears only in exceptional cases. The case of Convair throwing good money after bad on the 880 development rightly is regarded as an aberration, and the fact that General Dynamics had learned its style in military R and D undoubtedly was a contributing factor. But this kind of occurrence is the rule, not the exception, in military R and D. The B-58 and TFX were pushed all the way through development despite mounting unfavorable evidence. The B-70 and Skybolt were halted short of procurement but long after the signals were clear that they were bad ideas. It is a good bet that Boeing would not have persisted so long in pushing its swing wing SST design had the bulk of the funds been its own and had it the expectations of a market test against alternatives. I think the signals are clear enough that the

and W. H. Meckling, "Predictability of the Costs, Time, and Success of Development," and the Comment by F. M. Scherer on Klein's "The Decision Making Problem in Development," in *The Rate and Direction of Inventive Activity: Economic and Social Factors,* Universities-NBER Conference Series, Vol. 13, Princeton University Press for NBER, 1962. For a formalization, see my "Uncertainty, Learning, and the Economics of Parallel R & D Projects," *Review of Economics and Statistics,* November 1959. The discussion below is heavily compressed from a forthcoming paper by George Eads and myself.

present design is in trouble. It is the monopoly position and lack of pressure from an alternative that carries the project forward in its present conception. Similarly, throughout the history of the AEC's power reactor program, there have been complaints that the AEC was persisting in R and D on designs long after evidence had accumulated that this was not an attractive route, and conversely, that the AEC has been very sticky about initiating work on new concepts.

The problem transcends the likely inefficiency and high cost of innovation in industries where the mutation-selection model is not applied. These sectors are likely to end up with a far too limited range of choice and, further, with the government as a powerful lobbyist for the particular technologies. It is rather surprising that the producers of coal and oil, and of power-generating equipment using conventional fuels, have not raised more noise than they have regarding the pressure being applied to the utilities by the AEC to install nuclear rather than conventional power. The evidence on the nature of thermal pollution and nuclear waste problems now is far from clear. Even if it turns out that these problems are more amenable to solution than the pollution and waste problems created by the use of conventional fuels, nevertheless, I think we should feel some discomfort that a strong government lobby has a stake in the issue. There has been more vocal concern about the implications of a governmental financial stake in the SST, perhaps because of the explicit revenue-sharing provisions in the program. But even without a financial stake, the higher executives and congressmen who support the programs have a personal credibility stake in the success of the products and processes they push so hard. It is fairly clear that the success of the SST program, measured in almost any dimension that has been talked about, will depend greatly on the fare structure as allowed and encouraged by the Civil Aeronautics Board. The CAB can go a long way toward making the SST program a financial success by fighting for high fares (to cover the higher cost of the SST relative to the jumbo jets) and uniform fares (so that the lower-cost technology will not be able to compete in the dimension where it is strongest). These are the kinds of consequences one runs into, I suggest, when one tries to predict and plan innovation closely, rather than viewing the innovation process as one of mutation and selection.

The Problem of Achieving Dynamic Efficiency in the Public Sector

Earlier I made the point that the problem of efficiency in public sector activities is, in good part, a problem of industrial organization.

We economists have neglected this perspective before because we have been inattentive to the way that public goods or services are provided. Implicitly we have assumed that once the public decision was made (we spent a lot of attention on how that should be done) it was as good as effected. It now is clear that the public decision (even assuming there is such a clean-cut thing) has to be treated like a "demand" in the theory of industrial behavior, for the appropriate actions usually must be drawn forth from institutions—often some private as well as public—who cannot be assumed to jump simply because the President or a Cabinet Secretary says to jump; and very often the institutional structure provides the President or the public with limited alternative sources or with none; there is no real competitive mechanism.

The combination of the demand characteristics of public sector activities and the organizational structure of the sector apparently yield serious problems in a dynamic environment. I think most of us would agree that the dynamic performance of too large a fraction of the public and nonpublic sector has been extremely poor. While I have not collected any numbers and don't even know what numbers I should collect, my impression is that the average public sector batting average is much worse than the performance, on average, of sectors where the command and control mechanism is based on a real market for final products which links consumer satisfaction rather tightly to the profit or other success measures of the firms.

The problem is not characterized by inadequate research and development (although in some sectors, for example, education or urban services, this may be the case). In both defense and health there has been a lot of R and D, and technical change has been extremely rapid; but it also has been extremely expensive and poorly screened. My remarks above on the proclivity for expensive failures in defense research and development apply. In health one has the strong impression that one of the reasons for rising health costs has been the proclivity of doctors and hospitals to adopt almost any plausible new thing—drugs, surgical methods, equipment—that increases capability in any dimension (and some for which even that isn't clear) without regard to cost.

The basic problem appears to reside in the screening and spreading mechanism and seems inherent in a sector where for a variety of reasons full-blown consumer sovereignty is not possible or desirable and it is difficult to specify a set of clear-cut performance measures on which people can agree. Most of the traditional discussion, however, has been concerned with the characteristics of equilibrium positions. I would like

to argue that if the world is like Schumpeter's circular flow, one can conceive of a variety of mechanisms that ultimately can move the decision rules of a public or nonprofit firm toward those which reflect the public interest. The adjustment process clearly would be slow but it would get you there. Thus I am arguing that the serious problems of these feedback systems arise in a dynamic environment where change is occurring or is demanded.

How do we go about improving the performance of our educational system? The answer is not clear. Evidently we want to get more new approaches and programs tried out and evaluated. It seems plausible that the design and funding of major experiments should be undertaken at the federal level. But how does one really "evaluate"? Should success or failure be judged on the basis of how well children or their parents like the program? We long have been leary of putting too much weight on this for a variety of reasons. What objective scores are relevant? Clearly this is arguable. I maintain that with enough time and experimentation with a fixed number of alternatives, and easy modifications, it would be possible to get widespread agreement. But, I repeat, this takes time, and by the time we know how to evaluate the last block of alternatives we are faced with a new block of alternatives and conditions.

This point is salient in considering the new federal ventures toward educational reform. The nation clearly is beginning to put together the apparatus for running a lot of experiments, which seems to me an advance in how to generate an interesting spectrum of innovations. Two other proposed new departures recognize the problem of command and control over autonomous units, and cut at it from antithetical points of view. The educational voucher idea tries to build up the power of consumer sovereignty, and suffers from the variety of worries (alluded to above) we have about this. The performance contracting route attempts to increase the motivating power of those who think they can set objective standards, and indirectly to increase incentives to imitate the experimental programs that score well by these standards. However, the difficulties discussed above remain. As an in-between version one might well think of a voucher system, complemented by widely publicized evaluation of schools' performances, carried out according to the proposed relevant measures and intended to educate and inform parents. All of these are important structural changes. They clearly will help to make the system more responsive and progressive if we can solve the problem of evaluation, of distinguishing good departures from poor ones. But the "if" is basic, and the solution to this is not going to be easy.

These remarks were focused on education so that they would be specific, but I suggest they are applicable to a wide range of public and nonprofit sectors. I make them not because I have a solution, but rather in the hope that the appreciative theory of the problem may be useful, and because I think it extremely important that more economists be working on these problems.